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(Soviet Biology)

CHAPTER THREE

EMERGENCE OF MOLECULAR BIOLOGY

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Microbiology. Compared with other biological sciences, microbiology has been characterized by W. Braun in his text on bacterial genetics as "one of the last strongholds of Lamarckianism."[21] A panel appointed by the National Academy of Sciences (U.S.A.) explained the reasons for this as follows:

> Because of their biological material and their divorcement from the mainstream of genetic thinking, bacteriologists were often led to adopt a Lamarckian view of inheritance long after this model had been rejected by a majority of the biological community. Microbiologists had, for many years, been greatly impressed with the apparent genetic plasticity of microorganisms. It was, for example, easily possible to develop cultures that were resistant to various lethal agents simply by including them in the growth medium. Similarly, cultures capable of utilizing the sugar lactose could readily be derived from non-utilizers by passage through media in which lactose was the principal source of carbon and energy. It seemed that virtually any specific change desired could be brought about by introducing the proper agent. The feeling thus arose that these agents were causing instructional changes in the genetic makeup of the organism.... A series of ingenious experiments were performed to determine whether selective agents played any directive role in bacterial mutations. The first involved resistance to bacterial viruses and established that the resistant mutants found in a series of independent cultures grown in the absence of virus indicated that a heritable modification leading to resistance occurred randomly in the absence of the lethal selective agent. To such experiments were added others using the method of

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[21] W. Braun, Bacterial Genetics (W. B. Saunders: Philadelphia, 1953), p. 32.

> replica plating, which permitted the isolation and identification of pure resistant clones... from cultures that had never been exposed to the lethal agent. Bacteriology was no longer the last stronghold of Lamarckism.[22]

The experiments referred to above were those of S. Luria and M. Delbruck (1943) and of J. Lederberg and E. M. Lederberg (1952), but they did not totally dispel the view that some acquired characteristics in bacteria were heritable. As Edward Adelberg noted: "The Lederbergs' experiments convinced even the strongest supporters of the theory of directed variation the 'pre-adaptive' mutants exist, but not that such mutants account for all of the resistant cells which are revealed by a drug."[23]

Conclusive evidence was finally provided in 1955 by L. L. Cavalli-Sforza and J. Lederberg.[24] But even in the late 1950s such

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[22] Biology and the Future of Man, ed. by Philip Handler (Oxford University Press: New York, 1970), pp. 20, 22. The panel on molecular biology consisted of Sol Spiegelman (chairman), K. C. Atwood, Paul Berg, Edwin S. Lennox, Cyrus Levinthal, and Charles A. Thomas, Jr.

[23] Edward A. Adelberg, "Introduction", Papers on Bacterial Genetics (2nd ed.; Little, Brown and Co.: Boston, 1966), p. 2. The experiments referred to are: S. E. Luria & M. Delbruck, "Mutations of Bacteria From Virus Sensitivity to Virus Resistance", reprinted ibid., pp. 75-95, originally in Genetics, 1943, pp. 491-511; and J. Lederberg and E. M. Lederberg, "Replica plating and indirect selection of bacterial mutants", Journal of Bacteriology, 1952, p. 399.

[24] L. L. Cavalli-Sforza & Joshua Lederberg, "Isolation of Pre-Adaptive Mutants in Bacteria by Sib Selection", Genetics, 1956, pp. 367-381; reprinted in Papers on Bacterial Genetics, pp. 96-110.

Western workers as A. C. R. Dean and C. Hinshelwood believed and were attempting to prove experimentally that exposure to some factors in a culture medium could cause a "physiological reconstruction" within the cell which, if sufficiently prolonged, could result in heritable modification.[25]

Such opposition to the predominant views of bacterial geneticists was not extensive in the West, but its continued existence demonstrates that a belief in the possible inheritance of acquired or environmentally induced characteristics had intellectual roots in microbiology which had nothing to do with Lysenko's theories. The "expulsion" of Lamarckianism from Western microbiology, if it occurred at all, took place in the mid-1950s. Even today the scientific language used by microbiologists includes artifacts of these Lamarckian conceptions: "bacterial adaptation", "training" a culture, "inducing" resistance, "adaptive variation", and so forth.

These facts are helpful in understanding why bacterial genetics was not wholeheartedly embraced by many leading Soviet microbiologists. True, the development of Soviet bacterial genetics was not

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[25]A. C. R. Dean & C. Hinshelwood, "Aspects of the Problem of Drug Resistance in Bacteria", in Drug Resistance in Micro-Organisms: Mechanisms of Development, ed. by G. E. W. Wolstenholme & C. M. O'Conner (Little, Brown & Co.: Boston, 1957), p. 4. On this point, for the account of their work by a Soviet microbiologist who visited London in March, 1957, see N. D. Ierusalimskii, "Simpozium po adaptatsii mikroorganizmov k lekarstvennym veshchestvam v Anglii", Vestnik Akademii nauk SSSR, 1957, No. 7, pp. 58-61.

totally stymied: such geneticists as S. M. Gershenzon, S. I. Alikhanian, and V. L. Ryzhkov, who by the early 1940s had already begun work on bacterial or viral genetics, continued it in the 1950s in a variety of institutional settings.[26] But such work did not develop very extensively at the main Soviet center of microbiological research, the Academy's Institute of Microbiology, and the reasons can be traced in the writings of its director.[27]

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[26]S. M. Gershenzon worked in his laboratory of microbiology in the Institute of Zoology of the Ukrainian Academy of Sciences in Kiev; after 1966, this laboratory was expanded into a new Institute of Microbiology and Virology. S. I. Alikhanian headed a laboratory in the Institute of Antibiotics within the Ministry of Health and, after 1958, a laboratory in the Radiobiology Division of the Kurchatov Institute of Atomic Energy. V. L. Ryzhkov had been elected a corresponding member of the Academy in 1946. Beginning in 1961, their views appeared with greater frequency in Vestnik: see, for example, V. L. Ryzhkov, "Fiziologiia virusov i khimioterapiia virusnykh boleznei", Vestnik Akademii nauk SSSR, 1961, No. 8, pp. 59-63; and S. M. Gershenzon, "Molekuliarnye osnovy reproduktsii i izmenchivosti virusov", ibid., 1964, No. 11, pp. 71-77.

[27]On the work of the Institute during the period, see "Ob aktivnom izmenenii uslovii sredy mikroorganizmami", Vestnik Akademii nauk SSSR, 1955, No. 8, pp. 98-99; "V Institute mikrobiologii", ibid., pp. 78-79; "V Institute mikrobiologii", ibid., 1956, No. 8, pp. 90-91; "Ispol'zovanie mikrobiologicheskikh protsessov v promyshlennosti", ibid., 1957, No. 8, pp. 109-111; "Razvitie sovetskoi mikrobiologii", ibid., 1964, No. 7, pp. 111-113; and the annual reports appearing in Vestnik and Izvestiia Akademii nauk SSSR (biol. otd.) by Nesmeianov, Keldysh, Topchiev, Engel'gardt, and Sisakian. Such research did take place on a modest scale, however, especially in the laboratories of M. N. Meisel' and N. D. Ierusalemskii, but it only achieved proper scope after 1966 when N. D. Ierusalemskii was made director of the institute of microbiology and virology in the Pushchino complex.

Aleksandr Aleksandrovich Imshenetskii (1905-) became the director of the Institute of Microbiology in 1949 - months after the August session - and held the post at least through the late 1960s.[28] In that capacity, he attended many international conferences on microbiology and genetics, and made lecture tours in England and the United States.[29] He was also primarily responsible for articulating in Academy publications the successes and goals of Soviet microbiology.

His writings from 1955 through the early 1960s indicate almost no awareness of the importance of work on bacterial induction, transduction, and phage genetics. Concerning these, his article in 1955 on "The Longrange Future of Microbiology" contains only the following rather confused passage:

> This touches upon the method of the so-called "induction" of the properties of one species of microbe in another. Work of this sort is not always irreproachable methodologically, since the action of the living products of one species on another sometimes

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[28]On his career through 1967, see Aleksandr Aleksandrovich Imshenetskii, Akademiia nauk SSSR, Materialy k biobibliografii uchenykh SSSR (Nauka: Moscow, 1967).

[29]For reports on trips to Japan, England, Sweden, the United States, and Canada, see his articles "Geneticheskii simpozium v Iaponii", Vestnik Akademii nauk SSSR, 1957, No. 1, pp. 63-67; "V mikrobiologicheskikh uchrezhdeniiakh Anglii", ibid., 1958, No. 4, pp. 66-68; "Mezhdunarodnyi mikrobiologicheskii kongress", ibid., No. 11, pp. 69-72; "Mikrobiologicheskie issledovaniia v SShA", ibid., 1961, No. 6, pp. 72-75; and "VIII Mikrobiologicheskii kongress", ibid., 1963, No. 1, pp. 66-69.

> yields a new form corresponding to a third species. Hence, the specificity of these variations remains unproven. Frequently, such investigations relate to pathogenic microbes, in which species specificity rests on extraordinarily minute differences. In such species "fractions" the transition from one species to another has been achieved, but these transitions can sometimes occur even without any induction of properties as a result of the laboratory life of the microbe, i.e. by the action of the sum total of factors to which they are exposed in laboratory conditions. Only further profound study of the metabolism of microorganisms can lead to a mastery of the means to directionally vary their biochemical activity.[30]

This article, and in particular the passage cited above, were strongly criticized by R. L. Ryzhkov in a letter to the editors of Vestnik. Ryzhkov took issue with Imshenetskii for ignoring foreign work on recombination, transformation and the hereditary mechanism in bacteria and phage, results which "whether you like them or not...are perfectly precise, have been verified in a whole series of laboratories and constitute a fundamental revolution in our conceptions of the development of microbes...."[31]

In his article, Imshenetskii was expressing the views of a traditional microbiologist rather than of a bacterial geneticist: he dwelt

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[30]A. A. Imshenetskii, "O perspektivakh razvitiia mikrobiologii", Vestnik Akademii nauk SSSR, 1955, No. 6, p. 47.

[31]V. L. Ryzhkov, "Pis'mo v redaktsiiu: K voprosu o perspektivakh razvitiia mikrobiologii", ibid., 1956, No. 1, p. 123.

on problems of species definition, was impressed with the great variability and plasticity of bacteria, and believed their properties could be changed by factors in the environment, although he was vague as to the mechanism. In a report on a genetics symposium in Japan in 1956, he was highly skeptical of using such words as transduction, crossing-over, sexuality, genes and DNA in describing processes occurring between bacteria and phage, commenting: "There is not the slightest doubt that such conclusions and analogies have little foundation and are highly speculative in character."[32] But he continued: "Yet it would be incorrect because of this not to recognize the achievements of genetics, of which it is justly proud. The functional-morphological method for the study of the nucleus and chromosomes has brought forth very much of value...and we should develop these approaches more rapidly."[33] In his other writings, whenever he discussed such Western results, he usually emphasized the works of Dean and Hinshelwood, and continued to be skeptical not only of the validity

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[32]A. A. Imshenetskii, "Geneticheskii simpozium v Iaponii", ibid., 1957, No. 1, p. 67.

[33]Ibid.

of the results of bacterial genetics, but also of their usefulness.[34] One gets a feeling that he simply does not regard such work as the province of microbiology.

One of Imshenetskii's remarks is especially relevant to this point:

> Twenty years ago in a conversation with the author of the present article, an outstanding Soviet botanist expressed the view that the study of biological structures less than one tenth of a micron in size is already the task of the physicist, not of the biologist. Now, in connection with the problem of the localization of various enzymes inside the cell, the electron microscope is aiding the study of biochemical activity and morphology of granules which are substantially smaller.

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[34]On Hinshelwood, see A. A. Imshenetskii, "V mikrobiologicheskikh uchrezhdeniiakh v Anglii", ibid., 1958, No. 4, p. 67. On the lack of utility of such work, see A. A. Imshenetskii, "O nekotorykh zadachakh selektsii mikroorganizmov", ibid., 1959, No. 10, pp. 32-38; A. A. Imshenetskii, "Mikrobiologicheskie issledovaniia v SShA", ibid., 1961, No. 6, pp. 72-75, where he comments, "The connection between genetic investigations and microbial selection is very weak, and the theoretical bases of selection have been elaborated only poorly" (p. 75); A. A. Imshenetskii, "VIII Mikrobiologicheskii kongress", ibid., 1963, No. 1, pp. 66-69, where he comments, "However, from all those who entered into discussion on the importance of genetics in technical microbiology, it became perfectly clear that theoretical genetics, which has given so much to our understanding of the general laws of heredity and variability, is very far removed from the demands of practice" (p. 68); and "Aktual'nye zadachi mikrobiologii", ibid., 1963, No. 3, pp. 82-90.

> It would be incorrect to speak of the molecular biology of microbes, but it is possible to study the living processes of the microbial cell occurring on the molecular level, and it is being conducted along a broad front.[35]

Imshenetskii was not alone in these views. Similar motifs frequently appeared in the writings of other Soviet microbiologists during this period: skepticism towards Western results; belief in the inheritance of acquired bacterial characteristics; concern with species delineations; emphasis on the views of Dean and Hinshelwood when discussing Western work; and the tendency to view developments in bacterial and biochemical genetics as somehow not directly relevant to the practical tasks of microbiology.[36]

Such views obviously have a great similarity with those of Lysenko, whose theories emphasized the possibility of the inheritance of acquired characteristics by means of the "assimilation" by the organism of changed external conditions, occasionally making possible leaps from one species into another.[37] But it would be a mistake to

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[35]A. A. Imshenetskii, "Sovremennye zadachi obshchei mikrobiologii", ibid., 1961, No. 10, pp. 46-47.

[36]For example, see N. N. Zhukov-Verezhnikov, "O znachenii mikrobiologicheskikh dannykh dlia razrabotki teorii vidoobrazovaniia", Voprosy filosofii, 1957, No. 2, pp. 117-127; and N. N. Zhukov-Verezhnikov & A. P. Pekhov, "Genetika mikroorganizmov i sovremennye vzgliady na sushchnost' nasledstvennosti", ibid., 1958, No. 6, pp. 127-138.

[37]See, for example, T. D. Lysenko, Agrobiologiia (Moscow, 1952); "Novoe v nauke o biologicheskom vide", Botanicheskii zhurnal, 1953, No. 1, pp. 44-56; and "Teoreticheskie osnovy napravlennogo izmeneniia nasledstvennosti sel'sko-khoziaistvennykh rastenii", Pravda, January 29, 1963, pp. 3-4.

see the views of these microbiologists as identical to those of Lysenko, who in fact criticized what he regarded as their excessive Lamarckianism:

> The more I familiarize myself with the situation in this field of biology, the more obvious it becomes to me that many of our outstanding scientific workers in microbiology, even those who consider themselves partisans of Michurinist biology, have hitherto adopted positions with regard to the so-called adaptive variability of microorganisms which are essentially Lamarckian in nature.
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> It is well-known that Lamarckism is closer to the truth than Weismannism in all its old and new variants....But neither the earlier Lamarckism nor the current theory of adaptive variability of microorganisms can correctly explain the development of the living world....
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> Idealists and mechanicists in biology consider that the quality of hereditary variability does not depend on the quality of the factors acting. Hence they say that no sort of adaptive variability exists and that those forms and strains of microorganisms which are resistant to one or another antibiotic or medicinal substance are derived from non-resistant ones, not by means of adaptive variation, but by means of the selection and reproduction of resistant forms already present in the population....Against such arguments the partisans of Lamarckism are powerless....Why don't our microbiologists who consider themselves Michurinists maintain Michurinist positions on these questions?[38]

There is no need to dwell on Lysenko's "Michurinist" explanation, based on his law of "adequate variability" [adekvatnaia izmenchivost']

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[38]T. D. Lysenko, "Za materializm v biologii", Voprosy filosofii, 1958, No. 2, pp. 109-110.

but it is interesting that he regarded views such as those expressed by Imshenetskii as more Lamarckian than Michurinist. It thus seems likely that at least part of the resistance to molecular biology among Soviet microbiologists came from firmly rooted intellectual traditions of the discipline.

Biochemistry. Biochemistry played a greater role in the emergence of molecular biology in the Soviet Union than any other discipline. The field's importance can be seen from the following fact: from Lysenko's ascendancy in 1948 through the Academy re-organization of 1963, every Academic Secretary of the Division of Biological Sciences was a biochemist.

Following the 1948 session, Lysenko's supporter A. I. Oparin became the head of the division, and during the subsequent years he presided over the dissolution of genetics research within the division. In 1956, his colleague from the Institute of Biochemistry, V. A. Engel'gardt replaced him and worked actively to re-establish genetics and create conditions for the rapid development of Soviet molecular biology. Finally, from 1959 through 1963, their colleague N. M. Sisakian - also from the Institute of Biochemistry - presided over the division.

As we shall see, Engel'gardt's role is relatively unambiguous; he was a consistent and outspoken supporter of molecular biology and molecular genetics. The same cannot be said of Oparin and Sisakian, however. From 1948 through the mid-1950s, both strongly supported